Overdetermination and reference. Reply to Emma Ruttkamp

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OVERDETERMINATION AND REFERENCE

REPLY TO EMMA RUTTKAMP

A couple of papers deal with the two (almost entirely) overlapping chapters of ICR (5, 6) and SiS (7, 8) and one or more chapters from either ICR or SiS. However, only the paper by Emma Ruttkamp mainly deals with the topics of other chapters from ICR *and* SiS. Her main aim is to defend a kind of realism, called model-theoretic realism, that can make sense of the problem of *over*determination of theories by empirical data, using nonmonotonic ways of reasoning. Instead of going into details about her widely encompassing and intriguing approach, I would like to elaborate on two points that are directly related to her main themes, viz. the problem of overdetermination and the problem of reference of theoretical terms.

Underdetermination by Overdetermination

In Section 3 Ruttkamp suggests most of the time that the problem of overdetermination of theories by data is strongly related to the distinction between observational and theoretical terms, the O/T distinction, and the changing semantic relations between models, empirical reducts, and empirical models. However, in Note 6 she gives a formulation that makes clear that this problem is already present without the O/T distinction and without changing semantic relations. I would like to call attention to this basic version of the problem within my own framework in ICR. I will explain that, besides the traditional problem of underdetermination, due to theoretical terms that leave room for observationally equivalent theories, there is a more basic problem of determination operative in scientific research, a kind that can partly be conceived as a problem of overdetermination. In my ICR framework (see Section 7.3.2) the data are represented by $R(t)$, the set of realized possibilities up to t, i.e., *the accepted instances*, and by $S(t)$, *the strongest accepted law*, based on $R(t)$, where both are formulated within a previously chosen observational vocabulary. These data by

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no means determine a theory, let alone the strongest true (observational) theory T, corresponding to the set of nomic possibilities. Even if we restrict the attention to theories that are compatible with $R(t)$ and $S(t)$, that is, theories that can be represented as both a superset of $R(t)$ and a subset of $S(t)$, there will be, as a rule, many other theories besides T. Although by enlarging $R(t)$ and hence narrowing down $S(t)$ we zoom in on T in a two-sided way, normally speaking T remains underdetermined. However, $R(t)$ or, more precisely, the theory with $R(t)$ as its set of models, assuming that such a theory can be formulated, entails all the remaining theories "between $R(t)$ and $S(t)$," including T and many more. As a matter of fact this holds for any subset and even any member of $R(t)$. That is, after performing an experiment we can give a complete description of the realized physical possibility (relative to the observational vocabulary), which entails very many theories, including T itself. I am happy to agree with Ruttkamp's Note 6 that this is, in a sense, a problem of overdetermination.

A Problem of Reference

In her concluding section, following the five questions I put forward in the introductory chapter of ICR, it becomes particularly clear that Ruttkamp's model-theoretic realism and my constructive realism are close relatives. The main difference seems to lie in our view of reference. Although she does not criticize my analysis in ICR in detail, it is clear that she favors an epistemological kind of reference, whereas my basic analysis is semantic and metaphysical. Since I came to realize after closing ICR that I leave an important problem concerning reference open there in Ch. 9, I would like to take the opportunity to formulate this problem briefly. It will certainly suggest that the contrast with Ruttkamp's approach of reference be investigated further.

Let me start by quoting the most relevant summarizing claim in the concluding Chapter 13 of ICR (pp. 325-6):

> Now we arrive at a highly idealized picture of (new) research, in which we make the main metaphysical assumptions explicit. The scientist assumes the existence of two unconceptualized natural worlds, THE ACTUAL WORLD and THE NOMIC WORLD. THE ACTUAL WORLD includes its history, and its future, and is at least partially made by humans, among others, by scientists who perform experiments. THE NOMIC WORLD on the other hand, exists independently of human beings. It encompasses THE ACTUAL WORLD, and is to be studied via that world. Studying THE ACTUAL and THE NOMIC WORLD requires conceptualizing them.

The specific topic of reference (and ontology) is summarized on p. 329:

> Recall that in CHAPTER 9 we have defined 'reference' primarily in a 'domain and vocabulary' relative way, viz., in terms of the nomic truth generated by them and THE NOMIC WORLD, according to the Nomic Postulate. For attribute terms, the crucial question

> was whether the nomic truth is constrained by them; for entity terms, it was whether they occur as a domain-set of referring attribute terms. But we also suggested the possibility of basing on these definitions an absolute definition, viz., whether the term refers in at least one 'domain and vocabulary' combination. Note that the link with the nomic truth assures that reference may just be a potential matter, not (yet) actual, in the sense that the relevant nomic possibilities need not (yet) have been realized. In other words, terms always refer to THE NOMIC WORLD if they refer at all, and they may or may not refer to THE ACTUAL WORLD.
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> The corresponding ontology is roughly given by: entities and attributes exist as far as the corresponding terms refer. Note that the definitions are such that attributes only exist as far as there are entities having the attribute. Note also that, since reference is defined in terms of the nomic truth, there are again two kinds of existence, actual and potential.
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> To be sure, speaking of reference to, and existence in, THE NOMIC but not ACTUAL WORLD, is a way of speaking that has its risks. The more cautious way of speaking is to systematically talk about *potential* reference and existence.

As said, after closing ICR I came to understand that there is a problem with this way of dealing with reference. Whether a combination of an entity term and an attribute term refer, using a set of these (potential) entities as one of its domain-sets, will, in a context in which truth *approximation* is taken seriously, basically depend on whether something like these entities exists to which something like this attribute may or may not apply. However, what is "something like" in such a context? When do we say that there is nothing like that type of entity and that type of attribute, even apart from our probable lack of the epistemological means to apply the relevant terms? Maybe we should just take a formal point of view. As soon as the theoretical vocabulary introduces an entity and an attribute term they are supposed to be coupled to a combination of entities and an attribute "that are around" in the intended domain of application and that are not yet taken care of by the observational vocabulary. Of course, when more options are possible a choice will have to be made. I would like to conclude by conceding that these informal remarks still leave much to be desired.